Dear reviewer 2,

We thank the reviewer for his/her helpful suggestions, which led to significant improvements of our paper. Below we detailed how his/her comments are addressed in the revised version of the paper. The major corrections of the paper are cited here in *italic. We refer to specific pages by "P" and lines by "L". For example, "***P1, L1***" refers to page 1, line 1.*

(1) Insufficient treatment of radiative cooling term (RAD) quantification RAD is the dominant term controlling the convective overturning before the early morning, as also recognized by the authors. However, the equations (Eq. 2 and 3) used to quantify RAD in this study are too rough. As shown by Zheng et al. (2019), the RAD is most sensitive to two parameters: cloud optical thickness and moisture loading in the free atmosphere. If high clouds are present, the RAD will weaken significantly (e.g. Christensen et al., 2013). Even though the free-tropospheric moisture loading can be somewhat accounted for in Eq. (2) (the IWP), the cloud optical thickness and higher clouds can also modulate the RAD considerably. The blackbody assumption is only always valid for not-too-thick stratiform clouds (Zheng et al., 2019). The authors show that the RAD varies very little (_ 5 Wm-2), which could be artificial consequence of the two assumptions behind the equations (i.e. blackbody and no high clouds). Thus, given the significant role of RAD, it should be worthwhile to use a radiative transfer model instead. All inputs for the model are available from the observations: cloud-base and -top heights and soundings. Running it is computationally cheap.

We thank the reviewer for this valuable suggestion to use a radiative transfer code. However, the water or ice content, the base and the summit of each cloud layers is needed in the radiative transfer code in order to take into account the higher clouds effect. This information is missing for the DACCIWA campaign, since only integrated LWP, the LLSC base and top heights are available. So the use of the radiative code does not fully answer the reviewer comment. Despite this, the SBDART (Santa Barbara DISORT Atmospheric Radiative Transfer; Ricchiazzi et al., 1998) model is now used in our study to estimate the radiative cooling over the LLSC layer at the end of the stratus phase, based on radiosonde, ceilometer and cloud-radar measurements. The LLSC optical thickness is determined by a parameterized LWP. The higher clouds impact is partly taken into account through vertical profiles of temperature and relative humidity given by the radiosonde but an emissivity of clear air is applied to these thermodynamical characteristics. This limitation is further discussed in the paper. We obtain higher values (+ 15 W m^{-2} in average) of cloud-top radiative cooling than previously, but the standard deviation among the cases is still of 5 W m^{-2} and no difference can be noticed between coupled and decoupled LLSC.

The text was modified in several places to include the SBDART radiative code description, and the discussion of the results:

P11-12*: "The term RAD (Eq. 1.d) is retrieved from the vertical profiles of upwelling and downwelling radiative fluxes which are computed by using the Santa Barbara DISORT Atmospheric Radiative Transfer (SBDART) model (Ricchiazzi et al., 1998). This software tool, which solves the radiative transfer equation for a plane-parallel* *atmosphere in clear and cloudy conditions, was used in the studies of Babić et al. (2019a) and Adler et al. (2019) to estimate the temperature tendency due to radiative interactions during the LLSC diurnal cycle. For our simulations, the model configuration was very similar to that used in these studies. We prescribed 65 vertical input levels with a vertical resolution of 50 m below 2 km a.g.l, 200 m between 2 and 5 km a.g.l, and, 1 km above 5 km a.g.l. The vertical profiles of air pressure, temperature and water vapour density as well as the integrated water vapour are based on 05:00 UTC standard radiosounding data. The cloud optical thickness, which varies with its water and ice content, is required to describe a cloud layer in the SBDART model. Yet, the LWP provided by the microwave radiometer deployed at Savè supersite (Wieser et al., 2016) includes all the existing cloudy layers, and also is not available for five of our selected cases. Therefore, the LLSC optical thickness is determined from a parameterized LWP (Eq. 2), by assuming an adiabatic cloudy layer in which the liquid water mixing ratio (*q_l*) increases linearly (van der Dussen et al., 2014; Pedruzo-Bagazgoitia et al., 2020). The downwelling longwave radiations from potential mid-level and high-level clouds may reduce the radiative cooling at the stratocumulus top (e.g. Christensen et al., 2013). However, the cloud layers above the LLSC (base, top and water content) cannot be precisely described in the SBDART model from the available data set. Thus, the higher clouds radiative effect is not directly included in our estimate of downwelling radiative fluxes, but it is partially taken into account through vertical profiles of temperature and relative humidity given by the radiosonde. As the shortwave radiations are zero before the sunrise, only the longwave range, 4.5-42 µm with spectral resolution of 0.1µm (Babić et al., 2019a), was selected for radiative fluxes calculations. For all the cases, the vertical optical depth of ABL aerosol is fixed to 0.38, which corresponds to the average value of the measurements performed with a sun photometer in June and July 2016 at Savè.*"

(2) Inappropriate classification of the scenario of DD I am very reluctant to consider the clouds in Fig.10 c as "decoupled throughout the day". There are three possibilities for this case: (1) initially decoupled clouds remain decoupled and surface-heating driven cumulus clouds start to form underneath it. If they don't interact, the upper-layer clouds are decoupled and the bottom clouds are coupled; (2) if they interact, they form the cumulus-coupled stratocumulus-topped boundary layer such as those in downstream subtropical oceans; (3) If the initially decoupled clouds dissipate rapidly after decoupling, with only the underlying cumulus clouds left, this case is simply regular continental shallow cumulus that are, by definition, coupled.

All the above-stated cloud regimes are possible. Thus, it is a little bit misleading to call all of them "decoupled throughout". I would suggest either renaming it or adding additional discussions to clarify the definition of the decoupling.

We thank the reviewer for this comment. We fully agree that the three possibilities for scenario DD may occur. However, as stated in the paper, the scenario description is based on temporal changes of surface-based LCL and cloud base height measured by the ceilometer. From this point of view, in the scenario DD, the LLSC remains decoupled from the surface and thermally-driven (and coupled) shallow cumulus forms below it at the beginning of the convective phase. We are not able to test if the top of this underlying shallow cumulus interacts or not with the LLSC. So we kept the same name (DD) for this case. However, we completed the discussion about it.
The previous sentence "*In such conditions, the underlying cumulus clouds act to intermittently and locally couple the stratocumulus layer with the surface (Wood, 2012).*" was replaced by a more complete comment as suggested by the reviewer, ***P29, L24*** *: "In the case where the two cloud layers are superimposed, two possibilities may occur: (i) the underlying surface-convection driven cumulus cloud do not interact with the LLSC which remains decoupled from the surface, (ii) the underlying cumulus clouds develop vertically, reach the LLSC layer, and act to intermittently and locally couple it with the surface (Wood, 2012)."*
We moderated the statement in several sentences like this one, ***P30, L3***, *"One can wonder what conditions lead the LLSC to either be coupled to the surface in the scenario DC, or remains POSSIBLY decoupled with the formation of an underlying cumulus layer in the scenario DD."*
The previous sentence, in the Abstract, *"In the eight remaining cases, the stratiform cloud remains decoupled from the surface all along its life cycle."*, is now ***P2, L1:*** *"In the eight remaining cases, the stratiform cloud remains HYPOTHETICALLY decoupled from the surface all along its life cycle, since the cloud base remains separated from the condensation level."*

(3) Other comments: - Figure 2 and other figures: it should be helpful to use local time as well, which makes the readers easier to think of the problem from a diurnal cycle perspective.

We thank the reviewer for this suggestion. We indicate in the section 3, ***P7-L12***, that the local time at Savè, Benin is UTC +1 hour. In the revised version, this local time is repeated in the caption of Figures 2, 10 and 13.

- Page 10-11: some discussions on what determines the RAD is useful (check the work by Zheng et al., 2019).

We thank the reviewer for this comment. The radiative transfer across the stratocumulus layer is discussed in section 2; the text was modified to make it clear as follow, ***P5-L15***: *"During night-time, the longwave radiative cooling at the stratocumulus top is the leading process governing its maintenance. This cooling occurs because the cloud droplets emit more infrared radiation towards the free troposphere than they receive from the drier air above. It is modulated by cloud-top temperature, cloud optical thickness, thermodynamic and cloudy conditions in the free troposphere (Siems et al., 1993; Wood, 2012; Christensen et al., 2013; Zheng et al., 2019)."*

- Page 12, Line1: large-scale subsidence is commonly obtained from reanalysis data. Not very accurate, but better than nothing.

We agree with the reviewer and actually tried to use reanalysis data from the beginning. As mentioned in Pedruzo-Bagazgoitia et al. (2020), the large scale vertical velocity from reanalysis products present strong temporal and vertical variability, especially on early morning hours. We observed the same behaviour when we tried to use the ERA5 reanalysis products. Beside this, we observed a steady LLSC top at the end of the stratus phase in many cases. Consequently, we decided to use the Lilly (1968) assumption that implies the same order of magnitude between parameterized entrainment and subsidence velocities at the LLSC top.

The text is now, ***P12, L25***: *"For the term SUBS (Eq. 1.e), we have no possibility of estimating precisely the large scale subsidence at the LLSC top. One possibility is to consider evaluations from models or re-analyses. However, we decided to discard this approach, because the subsidence profiles from regional simulations with Consortium for Small-Scale Modelling (COSMO) or from ERA-interim and ERA-5 reanalyses showed a very high temporal variability and a strong lack of coherence among the different cases. According to the cloud-radar CTH estimates, the LLSC top is often stationary at the end of the stratus phases during DACCIWA. This feature has been observed (Adler et al., 2019; Babić et al., 2019a; Dione et al., 2019) but also simulated by Pedruzo-Bagazgoitia et al. (2020). Based on the LLSC top stationarity at the time of our LWP budget analysis,* $w_{s,CTH}$ *is estimated following Lilly (1968):"*

$$\frac{\partial CTH}{\partial t} = w_{s,CTH} + w_e \approx 0 \qquad (6)$$

"

- Section 4.1 as a whole: this section is centered on the difference between coupling and decoupling, however, what may cause the decoupling/coupling in the first place is not discussed in detail. There are several influential factors: cloud-top cooling itself (Nicholl 1984), precipitation (this is not important in your case), "deepening warming" decoupling (Bretherton and Wyant, 1997), and warm thermal advection (Zheng and Li, 2019). It may be more enlightening to discuss your results in the context of these potential influential controllers.

We thank the reviewer for this suggestion, and we hope to have improved the text. The section 4 has been deeply modified; a section 4.3 has been added, ***P26-27***, to discuss the results presented in section 4.1 and 4.2 about the relevant processes which are able to couple the LLSC during the stratus phase. In summary, none of these processes was clearly pointed out as responsible for the coupling during this phase and a combination of several of them, each with a small effect, should be considered.

- Page 22, Line 15: again, it could be due to too simple treatment of RAD.

We do agree with the reviewer. The use of SBDART certainly gives a better treatment of RAD but still not complete, since the higher clouds are not fully taken into account. This is discussed in the revised version ***P24, L3***:

"We find only a 5 W m^{-2} standard deviation for the radiative cooling at the LLSC top and no particular difference between cases C and D. This very low standard deviation may be due to the conditions which remained very steady from one case to the other, but may also be underestimated because the higher clouds impact is not fully included in the radiative fluxes estimate. In order to evaluate the error due to the temperature underestimation above the LLSC top, SBDART is run with the measured and a corrected temperature profile, while the other inputs remain unchanged. The correction of the potential temperature vertical profile consists in a linear tendency between the measured θ plus a 1.2K correction right above the CTH, and the measured θ at 800 m, where we consider that the radiosonde sensor is no more affected by the cloud crossing. The cloud-top radiative cooling estimated by SBDART with this corrected temperature vertical profile is larger by less than 2 W m^{-2}."

- Figure 13: there are too many symbols, making the readers hard to recognize each of them. This defeats the purpose of using a diagram for illustrations. Try to use process-based cartoons (e.g. the one from Wood 2012).

We thank the reviewer for this valuable suggestion. Process-based cartoons are now used in Figure 13 to illustrate the different scenarios, ***P37***.

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